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unique in themselves which serve to illuminate general biological principles. One can not for a moment doubt that in some way or other the processes of existence have worked striking modifications in the parasitic forms when one compares them with their free-living relatives and scans the changes which take place in the individual development. Not even the most extreme conservative would wish to maintain that the parasite was an original product, or would hesitate to grant that it had become adapted to its present mode of life. Yet more extraordinary and far-reaching structural modifications could hardly be asked for or found than those which are evident in the parasitic organisms. Nor would it be easy to conceive more intricate or more precisely balanced relations than those which exist between some parasites and their hosts. The development, modifications and habits of the parasite have been coordinated with the conditions of existence in the host in strikingly precise fashion. Investigation has as yet only begun to work out the adjustments which have arisen independently and in great variety in different species and groups of parasitic species. The field is one that offers unusual opportunities at the present time to the investigator.

We do not know how far an intimate study of these problems may carry us towards the explanation of the process of evolution in free-living organisms. There is reason to think that the change has been more rapid as well as more radical among parasitic species. And if so, the study of this problem at this point may be expected to throw welcome light on the factors that lead to structural changes in living organisms. Any such study will certainly serve an important purpose in broadening the human mind and encouraging it to seek the

solution of the problems of existence more vigorously than it has even done as yet.

The outlook for the future constitutes no less than the achievements of the past, a real contribution to the cause of human progress.

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THE INDIGEN AND CULTIGEN

If an author were to prepare a flora or manual of cultivated plants in any country, he would come hard against the fact that he deals with two gentes or types of species.

One gens has recorded origin, with the typical form well recognized and probably represented by a "type specimen" in the herbarium of the person who "founded" the species. It in an indigen of known habitat.

The other gens is a domesticated group of which the origin may be unknown or indefinite, which has such characters as to separate it from known indigens, and which is probably not represented by any type specimen or exact description, having therefore no clear taxonomic beginning. I trust I may be pardoned for calling such species or group a cultigen.

A good example of the cultivated indigen is *Thuja occidentalis*. Although there are many horticultural forms, their relationship is understood, we are familiar with the species in the wild, and we have the whole case before us. The variations under domestication are indeed great, but we readily range them with what we call the species itself.

A good example of the cultigen is *Zea Mays*. We know neither its country nor its origin. It is widely variable. If a botanist had before him good material of all these variations, I do not know what one of them he would take as "the type." It is a composite gens, with no clear taxonomic center from which variations diverge.

Here we have two classes of facts, with no adequate way of expressing one of them in taxonomy.

If *Zea Mays* were an isolated case we could treat it as an exception. I have before me a list of one or two hundred comparable cases,

and yet I have made no careful search. Botanical literature is full of cultigens, improperly or incompletely coordinated into taxonomic treatment.

The prime deficiency is the fact that many of the good cultigens are unrecognized botanically. In the presumed manual of cultivated plants, how would the author treat the tuberous begonia? Would he enter descriptions of the several indigenous species from which the cultigenous group has come, and stop there? But what, then, would the horticulturist do? He would say that *Begonia Veitchii*, *B. rosæflora*, *B. Davisii*, *B. Pearcei*, *B. Clarkei* are not in cultivation so far as he knows, and he asks what he shall call the tuberous begonia. He would charge that the tuberous begonia is left out, and his statement would be correct. If he is a dealer, he naturally and properly wants a name in his catalogue comparable with *Begonia Rex* and *B. semperflorens*. Voss solves this problem by calling the cultivated group *B. tuberhybrida*.

Now the botanist will say that *Begonia tuberhybrida* has no "type," no clear description properly published, and therefore no recognized taxonomic standing. It is essentially as good a case, however (except traditionally), as *Zea Mays*, which some persons now consider to be a bigeneric hybrid.

If we accept the Linnean and other historic cultigens, why not accept modern groups of similar or comparable origins? Are the following "good species" in the strict sense? *Triticum vulgare* L., *Hordeum vulgare* L., *Secale cereale* L., *Helianthus annuus* L., *Saccharum officinarum* L., *Pyrus Malus* L., *Ipomoea Batatas* Poir., *Abutilon pleniflorum* N. E. Br., *Lonicera americana* Koch, *Lilium japonicum* Thunb. and *L. testaceum* Lindl., and any number more. We have similar cases in the domestic animals, as *Felis domesticata*, *Gallus domesticus*, *Canis familiaris*.

What are we to do with cultivated blackberries, ixias, gladiolus, fuchsias, and many of the magnolias, deutzias, spireas, pandanus, roses? What are we to do with the cultivated canna: what is this plant? Are we merely to pass it by, undescribed because it is a com-

plex? To describe the various species of canna is of no consequence to its identification. At present there is no name under which we can describe the common garden canna. The point is, are we to name and describe cultivated plants or are we not?

What are we to do with such things as *Saintpaulia kewensis*, *Tritonia crocosmaeflora*, *Iris flavescens*, *Ligustrum coriaceum*, *Eryngium Oliverianum*, *Fuchsia speciosa*, *Heuchera brizoides*, *Primula Polyantha*?

The cultigens are with us, and the numbers will increase. No longer can we let them go by default. The plant-breeder will bring his new groups; will taxonomy expand itself to receive them, or must they always be outcasts?

Even when the parent indigen is known, many of these cultigens have their own entity and by every taxonomic right should be separately recognized. They often present characters new, or different from those of the fundamental species, or at least in different combination. When recognized as admissible gentes, in the company of living things, they are no longer involved in debates as to the taxonomic merits of their ancestors. Even if we were satisfied to say that the cultivated blackberries are *Rubus allegheniensis*, what are we to say when *R. allegheniensis* is itself split into a dozen segregates?

Suppose, now, we are to agree that *Zea Mays* is a hybrid of *Euchlæna mexicana* and *X*: are we then to describe *E. mexicana* and *X* in our manuals, and to say that Indian corn is a hybrid between them, dropping the name *Zea Mays* entirely? This is exactly the type of treatment we are giving great numbers of cultigens that have well-marked characteristics of their own. Many of our common cultivated plants can not be put in our manuals, because we have no names to call them by. What are we to call the florist's chrysanthemum? To describe its supposed parents, *C. indicum* and *C. morifolium*, is of no consequence; these are unknown to the cultivator, and moreover they are not the florist's chrysanthemum.

It is said that to admit such forms into the society of recognized species would greatly

disturb systematic botanical procedure. The replies to this position are two: (1) We have already admitted very many of them, even if under protest in some cases; (2) is botanical procedure to be competent to accept the facts of nature? Whether we will or no, these cultivated things will be known by botanical names. What are we to do with *Phlox decussata*? It may be a set of hybrids between *P. paniculata* and *P. maculata*, but we can not order the plant from the nurseries under either of these names. Referring the name *P. decussata* to one or the other of the species may satisfy the demands of synonymy, but it does not dispose of the plant. It is a good name for the group: why not use it?

Naturally we must have a formal and recognized system of taxonomy and nomenclature. We should keep it pure. But may it not be extensible? The interminable discussions over trivialities of priority in nomenclature tend to seal up the subject as a closed book, or as an ancient box of precious ointments. May we not open the book or carefully lift the lid?

I have no program. To-day I am only asking questions. I would not interfere in any way with the orderly procedure that we have found to be good. I would disturb nothing: but may we add?

May we not admit the cultigen, under well-considered practise of conservative and trained botanists, defended with proper safeguards? I am not thinking of mere variations, even if well marked, but of important groups or clans of known characteristics under domestication. If so, the gens should have standing, which means that the name should bear record of its author. Its name should have recognized botanical form, for cultigens are still plants and of more or less coordinate rank with other gentes known as species. While falling under recognized botanical procedure, might it not represent a category or class of its own? In the manuals perhaps its name would be set in a different type; or could a designating symbol be used? Under the International Rules, the cross-mark (x) preceding the name is recommended to distinguish

hybrids; this can not be applied to any extent because records of hybrid origins are few; it does not touch the great class of cultigens: yet there must be some good way of distinguishing categories.

We must assuredly try to avoid confusion, but we do not accomplish this by avoiding the facts. Horticulturists as well as botanists are entitled to protection and precision. May we not make names for certain cultigens?

These may be troublesome questions, but they force themselves on us. Is it not best to meet them squarely, and provide a way?

If we can not modify our practise in these regards, there is no use of making a manual of cultivated plants.

L. H. BAILEY

SCIENTIFIC EVENTS

THE RESEARCH COMMITTEES OF THE BRITISH INSTITUTION OF MECHANICAL ENGINEERS

SOME particulars of the work of the research committees formed under the direction of the Institution of Mechanical Engineers are given in the report of the council for 1918 and are quoted in the London *Times*.

The alloys research committee has been occupied with investigations on various light ternary alloys. These investigations have been conducted at the National Physical Laboratory with the assistance of the Department of Scientific and Industrial Research from whom the committee received a grant of £400, in addition to £800 paid directly to the laboratory for the provision of special plant. The council of the institution made a grant of £250 for the year. The committee's eleventh report, which would have contained the results of these investigations, has been temporarily withheld in the public interest.

The committee on steam nozzles, which received a grant of £100 from the council, has been so fully occupied with war work that it has been unable to construct apparatus and carry out tests; but complete detailed working drawings of the apparatus for measuring the impulse of steam jets have been prepared, and it is hoped that the apparatus may shortly be put in hand.

Dr. Stanton, with his special machine at the